(*b*) Exchange of information between the directors concerning the subjects under investigation at the respective laboratories, with the view to prevent duplication of work, but particularly to advantageously supplement at one laboratory work which in some of its phases may be under way at the other. For instance, certain work at the Marine Biological Laboratory may have economic connections which would not be given much consideration. Probably an investigator at the Fisheries Laboratory could be assigned to this side of the subject to the mutual advantage of both workers, economy of material and effectiveness of effort. Conversely, while the Fisheries Laboratory is concerned with investigations more directly related to the fishing industry, there frequently arise in connection with them collateral, more abstract, problems which would perhaps appeal to investigators at the Marine Biological Laboratory.

(*c*) Reciprocal access to daily collections. It frequently occurs that when no one at a laboratory has an interest in a certain organism, or classes of organisms, the material collected is either thrown away or imperfectly cared for. If when the collections are brought in a competent person from the other laboratory, and familiar with its needs, could be given an opportunity to examine the collections, or at least the rejected material, much now wasted might be utilized.

(*d*) The effectiveness of the collecting could probably be increased by such cooperation as would prevent duplication in the fields covered. This could be arranged by an understanding of mutual requirements and the cooperation of the collectors.

I share the feeling entertained by many others that a new era in American biological science is now dawning; and that, under the inspiration and stimulus afforded by Mr. Crane's noble gift, the day is not far distant when Woods Hole will come to be generally recognized abroad as well as at home as the world's biological Mecca.

Hugh M. Smith

TIME RATIOS IN THE EVOLUTION OF MAMMALIAN PHYLA. A CONTRIBUTION TO THE PROBLEM OF THE AGE OF THE EARTH

Considered as a historic science, geology has not yet solved its first problem. There is as yet no satisfactory way of estimating the age of the earth and the length of geologic periods. The various methods that have been devised to compute it are all subject to such large factors of uncertainty dependent upon questionable assumptions, that the most that can be claimed for them is that they indicate the order of figures which should be assigned as the antiquity of geologic periods. The relative length of the periods one with another can usually be more definitely gauged. But the translation into years is a matter of wide divergence of opinion and no real proof that any of the results are even approximately correct.

It is quite true that various estimates have been made by geologists and physicists resulting in figures which are of the same order of magnitude and in reasonably close agreement, although derived from independent sources. This might be taken as evidence that the age probably lies within these limits. But in fact it does not prove any such thing, for it rests in every case upon the assumption that the activities, whose accumulated results are the measure of the length of time that they have been in action, have proceeded in past times at the same pace as at present. This is not only unproved, there are strong reasons for believing it widely different from the fact.

There is no occasion to review these methods of computation or to point out other unprovable assumptions. Every competent discussion of the subject has sufficiently called attention to them.

What I have to contribute is the suggestion of a possible measure derived not from inorganic, but from organic evolution. It is approximate indeed, and relative, based like the others upon assumptions which can not be proven. But it is perhaps—I dare not say more—free or partially free from subjection to the varying intensity of inorganic activities

which vitiates in common all calculations based upon the assumption of their constancy.

In working upon the numerous phyla of vertebrate animals, especially of mammals whose evolution is recorded in our Western Tertiaries, I have been impressed with the fact that they seem to have a fairly constant maximum rate of progressive evolution. The rate of alteration in structures that are being changed adaptively to some changing environment or habit is fairly uniform, comparing one phylum with another. Where concentrated upon one element of change or a few, it is more rapid; when distributed into a great number of alterations of a complex structure it is slow. Some structures are much slower to change than others—notably this is true of the teeth as compared with the bones of the skeleton.

It is essentially a constant progressive change. Where we find sudden jumps of any considerable magnitude the explanation is always at hand, and usually obvious when the circumstances are studied judicially, that we are dealing with an imperfect record, and the breaks are due to migration or to unrecorded lapse of time. To prove this point—a disputed one, I am well aware—would take me too far afield. I must rest on the assertion that twenty years study, in field and laboratory, of American fossil mammals, has brought me to the conclusion that the evolution of their phyla took place through the cumulation of minute increments of structural change, at a rate which, whether concentrated upon one feature or distributed over many, presents some approach to a uniform maximum.

I fully believe that the change is due to the pressure of the environment, acting through selection upon individual variations. Whether these be mendelian or fluctuative in their law of transmission is immaterial. The point is that they are minute, well within the limits of a species as conservative paleontologists draw those limits.

If they are accumulated through selective action of the environment, how can they be said to be in any sense free from the varying rate of change of inorganic activities which vitiate calculations based upon the constancy of their action. If the environment is changing rapidly at one time, slowly at another, will not this be reflected in the rate of change of any phylum of living beings? Undeniably this is true. Yet there does appear to be a maximum rate of change as above outlined, and environmental change exceeding that limit results in migration and extinction, not in structural alteration. Moreover, a large part of the structural evolution which we can observe must be in reaction to the pressure of the biotic, not the physical environment. A large portion of the progressive structural change is advantageous to the animal under any circumstances, whether or not the physical environment changes. This is peculiarly true of increase in brain capacity; it is partly true of increase in mechanical perfection of the structure leading to increased speed, better tooth mechanism as well as numerous changes not recorded in the skeleton.

It would seem therefore that there is a maximum rate at which alterations in the structure can take place. I suppose this rate to be conditioned by two factors, individual variability in the organism, and selective processes under the conditions obtaining in nature. At all events the fact stands as of record, proved and confirmed by innumerable instances, that the evolution of any direct phylum does take place through cumulation of minute changes, at a rate which, allowing for concentration upon one element of change or dispersal over many, does present a considerable degree of uniformity in corresponding parts, whether of the same phylum at different times or of different phyla at the same time. This rate may often not be attained, but I can find no convincing evidence that it can be exceeded.

The amount, variety and fundamental character of the differences thus accumulated are the practical measure of our systematic classification. A difference or group of differences of small amount, yet distinctly beyond the limits of individual variation, is customarily regarded as specific. Differences of a decidedly larger order are considered generic, and so on. It would perhaps be a fair average estimate

to say that one genus differs from the next ten times as widely or fundamentally as one species from its next neighbor. There is no sort of exact rule in the matter, but this would perhaps represent the average opinion to which each systematist endeavors to conform in arranging the group upon which he is working.

Now if the above conclusions are warranted we may find in the recorded evolution of various well-known phyla a rough measure of the relative length of the epochs covered by its evolution. In instance we may take the evolution of the horse. This phylum as represented in the American Tertiaries I believe to be a direct phylum so far as the genera are concerned; the relation of the species to the direct line of descent are mostly immaterial to the present discussion.

Equidæ (Direct Phylum)	Relative Amount of Structural Difference from Preceding Stage	Geologic Epochs
Equus caballus, etc.	1	Recent
Equus scotti, etc.	10	Pleistocene
Hipparion	10	Pliocene
Merychippus	15	Miocene
Parahippus	5	
Miohippus	5	Oligocene
Mesohippus	15	
Epihippus	10	
Orohippus	10	Eocene
Eohippus		
		Paleocene

It would be possible to verify these estimates of structural differences by comparative measurements. But it would be an enormous task. To select a few of the great number of structural differences for measurement would be almost certainly misleading; to average them all would entail many thousands of measurements for each species or genus compared. The final result might be twice as much or half as much as the estimate I have given; it would certainly not be ten times or one tenth as great. The margin of error for each estimate here given is not to any great extent cumulative for the whole series. The errors would therefore tend to balance to some extent, and the margin of error for the whole series would be less in proportion. For these reasons, and because of the doubt already expressed as to whether the maximum rate of evolution is really a constant, I have not thought it worth while to verify the estimates by measurements.

From the beginning of the Pleistocene to the present time, the evolutionary change in the phylum is measured by the difference between the modern species and the nearly allied species found in the Aftonian and other equivalent formations of early interglacial time. During the Pleistocene there has been a great deal of migration and shifting of faunas; the actual evolutionary change in this or any other mammalian phylum is notably small. It is perhaps one tenth the amount of structural change that separates *Equus* from *Hipparion* of the late Miocene and early Pliocene. *Hipparion* in turn differs about as much from *Merychippus* as it does from *Equus;* the estimated structural difference between the earlier stages is represented by the remaining figures in the column. Adding up these figures, we find that the amount of structural change in the *Equus phylum* during the Tertiary is 85 times the amount of Pleistocene evolution. So far as this is a measure of geologic time, it means that the Tertiary from Suessonian upward was 85 times as long a period as the Pleistocene. To this should be added a considerable figure for the Paleocene, whose length based on the evolution of other phyla might be assumed at 10 or 15 times the length of the Pleistocene. Briefly then, on this basis we should assume that the entire Tertiary is about 100 times as long as the Pleistocene, dating the latter from the first great glacial advance.

This is greatly in excess of the proportion usually assigned. But the Pleistocene was a time of extreme activity in sedimentation, denudation and other inorganic activities whose rate affords the basis of the various calculations that have been made. The amount of Pleistocene denudation, the thickness of its sediments, would hence give a greatly exaggerated measure of its length in time as compared with the whole of the Cenozoic.

The various other phyla of mammals support these proportions fairly closely. None are

quite so complete, direct and obvious in their structural change as the Equidæ. But the results obtained by a careful consideration of the phyla of Camelidæ, Rhinoceridæ, Tapiridæ, Canidæ, etc., do not appear to me to differ materially.

It is only in a very general and tentative way that we can apply these standards to the Mesozoic. A comparison of the amount of evolution in vertebrates between the end of the Permian and the end of the Cretaceous in comparison with the maximum change from the end of the Cretaceous to the present day, gives in turn the impression of a distinctly higher order and more fundamental quality of change. My impression would be that each of its four periods, Triassic, Jurassic, Comanchic, Cretacic witnessed structural changes in vertebrate phyla as extensive and profound as those that took place in the Mammalian phyla during the Tertiary. As to the Palæzoic, I have no basis for an opinion. It should be remembered that it is the maximum rate of change that is used as a measure. Many races, more often many characters in a race, changed slowly or not perceptibly.

It will be obvious that, if these proportions hold true, an estimate of the length of the Pleistocene will afford a measure of the length of the Tertiary and older periods in years. But the estimates of Pleistocene time differ enormously. The lowest estimate is perhaps by G. F. Wright, who will not allow more than 25,000 years. At the other extreme stand Penck and other authorities with estimates of 1,500,000 years or more. The more moderate figures of 50,000 to 200,000 years generally adopted seem more probable than either extreme. According to the proportions above estimated of Tertiary to Pleistocene time, we should have

Pleistocene	Tertiary	Mezozoic
25,500 years (Wright)	$2\frac{1}{2}$ million	10 million
100,000 years (Walcott)	10 million	40 million
1,500,000 years (Penck)	150 million	600 million

If the proportions usually assigned to the Paleozoic be correct, it was as long as or longer than Mesozoic and Tertiary combined. This would give twenty-five million years for the whole of the fossiliferous record upon the extreme figures of Professor Wright; on Walcott's estimate over 100 million, and on Penck's over 1,500 million years. For various reasons I am disposed to believe that the relative length of the Paleozoic should be revised upward, but the estimate of ten million years for the Tertiary and forty for the Mesozoic does not seem unreasonable.

W. D. MATTHEW

AMERICAN MUSEUM OF NATURAL HISTORY

SCIENTIFIC NOTES AND NEWS

AMONG the large numbers of American scientific men and university professors now detained on the continent and in England, probably the most serious inconvenience is suffered by the surgeons who attended the recent congress in London, some nine hundred of whom are said to be unable to obtain passage home. The only serious difficulty so far reported is the arrest and imprisonment of Mr. and Mrs. Archer M. Huntington in Nuremburg, Bavaria. Mr. Huntington is president of the American Geographical Society, and it is said was making a study of aeronautical routes.

PROFESSOR ELIE METCHNIKOFF, assistant director of the Institut Pasteur, will next year celebrate his seventieth birthday and the fiftieth anniversary of his doctorate. A committee has been formed, under the presidency of Dr. Roux, director of the Institut Pasteur, for the celebration of the anniversary which will include the publication of a "Festschrift."

MR. MARCONI has had the order of the Honorary Grand Cross of the Victorian Order conferred upon him.

AMONG those upon whom the University of Aberdeen conferred honorary degrees at the recent meeting of the British Medical Association were Mr. W. T. Hayward, Mr. T. J. Verrall, Sir Victor Horsley, Dr. Archibald Garrod and Sir John Bland-Sutton.

THE first presentation of the Saville medal, established by the West End Hospital of Nervous Diseases, London, in memory of the late